# VII. *An Account of a Book intituled,* New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Inveſtigation of the reſiſting Power of the Air to ſwift and ſlow Motions; by *B. R.* F. R. S. as far as the ſame relates to the Force of Gunpowder.

*Read* April 14. and 21. 1743.

THIS Treatiſe contains Two Chapters. The Firſt treats of the Force of Gunpowder, and the Velocities communicated to Bullets by its Exploſion: The Second conſiders the Reſiſtance of the Air to Bullets and Shells moving with great Velocities; and endeavours to evince, that this Reſiſtance is much beyond what it is generally eſteemed to be; and conſequently that the Tract deſcribed by the Flight of theſe Projectiles, is very different from what is uſually ſuppoſed by the modern Writers on this Subject.

The principal Points endeavoured to be eſtabliſhed in the Firſt Chapter are theſe, "That the Force of "fired Gunpowder is no more than the Action of a "permanent elaſtic Fluid, which is produced by the "Exploſion; that this Fluid obſerves the ſame Laws "with common Air in their Exertion of its Preſſure "or Elaſticity;" and conſequently, "That the Ve-"locities communicated to Bullets by the Exploſion "may be eaſily computed from the common Rules, "which are eſtabliſhed for the Determination of the "Air's Elaſticity."

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The Two firſt Propoſitions contain the Proofs that a permanent elaſtic Fluid is conſtantly generated in the Exploſion of Gunpowder; this is evinced by well-known Experiments daily repeated, and acquieſced in by all who have frequented the uſual Courſes of Experimental Philoſophy, of which theſe Experiments generally make a Part; ſo that the Author preſumes he may conſider this Point as inconteſtably eſtabliſhed, at leaſt he has never yet met with any who have queſtioned it.

The Third Propoſition is, That the Elaſticity of this Fluid produced by the Firing of Gunpowder, is, *cæteris paribus*, directly as its Denſity; and the Experiment by which this was confirmed, was letting fall ſeparately Two Quantities of Powder, the one double the other, on a red-hot Iron included in an exhauſted Receiver; and it appeared by the Deſcent of the Mercury, that the Elaſticity of the Fluid produced from the double Quantity of Powder, was nearly double the Elaſticity of that produced from the ſingle Quantity; that is, the Elaſticity was nearly as the Denſity of the Fluid.

But it may perhaps be thought, that a ſingle Experiment is too ſlender a Foundation on which to build ſo material a Principle, ſince all ſubſequent Reaſonings on the Force of Powder in ſome meaſure depend on it. In Reply to this it may be ſaid, that the Author recited this ſingle Experiment on account of the great Quantity of Powder made uſe of in it, which was Three-ſixteenths of an Ounce; but that he had really made many more equally concluſive, which he thought it unneceſſary to mention. However, thoſe who doubt of this Propoſition, may

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ſatisfy themſelves herein by ſome Experiments made by the late Mr. *Hauksbee* before this SOCIETY, though with a different View; where, by the firing of Twenty-ſix Quantities of Powder ſucceſſively, the mercurial Gage was ſunk from Twenty-nine Inches and an half, to Twelve Three-fourths; for by comparing theſe Experiments together, and making the neceſſary Allowances, it will be found, that the Elaſticity was nearly proportional to the Denſity in all that Variety of Denſities.

In this Propoſition, the Analogy between the Fluid produced by the Exploſion of Powder and common Air, is eſtabliſhed thus far, that they exert equal Elaſticities in like Circumſtances; for this Variation of the Elaſticity, in proportion to the Denſity, is a well-known Property of common Air. But other Authors, who, ſince the Time of Mr. *Boyle*, have examined the factitious elaſtic Fluids produced by Burning, Diſtillation, *&c.* have carried this Analogy much farther, and have ſuppoſed theſe Fluids to be real Air, enduced with all the Properties of that we breathe; particularly the Reverend Dr. *Hales*, who has purſued this Examination with the greateſt Exactneſs, in a Series of the beſt contrived Proceſſes, conſtantly affixes the Denomination of Air to theſe factitious Fluids, he having found, that their Weight is the ſame with that of common Air, and that they dilate with Heat, and contract with Cold; and that they vary their Denſities under different Degrees of Impreſſion in the ſame Proportion with common Air; and from hence, and other Circumſtances of Agreement between them, he ſuppoſes them to be of

the ſame Nature with Air, and conceives them to be fitly deſigned by the ſame Name.

But ſo perfect a Congruity between theſe factitious Fluids and Air is not neceſſary for the Purpoſes of this Treatiſe. The fundamental Poſitions of this Firſt Chapter ſuppoſing no more, than that the Elaſticity of the Fluid produced in the Exploſion of Gunpowder is always, *cæteris paribus*, as its Denſity; and that the Force of fired Gunpowder is only the Action of that Fluid modified according to this Law. It has been already mentioned, on what Grounds the Firſt of theſe Principles hath been aſſerted, as contained in the Third Propoſition; and it remains to explain the Reaſons urged for the Support of the laſt in the Eight ſucceeding Propoſitions.

The Law of the Action of this Fluid being determined, Two Methods offer themſelves for inveſtigating the abſolute Force of Powder on the Bodies it impels before it. The firſt by examining the Quantity of this Fluid produced by a given Quantity of Powder, and thence finding its Elaſticity at the Inſtant of the Exploſion; the other by determining the actual Velocities communicated to Bullets by known Charges, acting through Barrels of different Dimenſions. The Firſt is the moſt eaſy and obvious, but the Second the moſt accurate Method; and therefore the Author has ſeparately purſued each, and he has found, that their Concurrence has greatly exceeded his Expectation, and thereby both of them receive an additional Confirmation.

The Quantity of the elaſtic Fluid, produced by the Firing of a given Quantity of Powder, is determined by

by firing it in an exhauſted Receiver, and obſerving how much the mercurial Gage ſubſides thereby, making a proper Allowance for the Increaſe of its Elaſticity from the Heat of the included hot Iron. But then, as the Subſiding of the Mercury is not meaſured till the Flame of the Powder is extinguiſhed, and the Fluid is reduced ſomewhat near the Temperature of the external Air, it is evident, that the Elaſticity thus eſtimated is much ſhort of what it really was in the Inſtant of Exploſion; and therefore, to obtain that Elaſticity, which is the Force ſought, it is neceſſary to make ſome Eſtimate of the Increaſe of the Elaſticity of the Fluid by the Fire and Flame of the Exploſion. For this Purpoſe it is examined in the Fifth Propoſition, how much the Elaſticity of common Air is increaſed by a Degree of Heat equal to that of Iron beginning to grow white hot; and it is found, at a Medium, to be thereby augmented ſomething more than Four times; whence, as the Fluid produced by any Quantity of Gunpowder takes up, when compreſſed by the Weight of the incumbent Atmoſphere, a Space ſomething leſs than 250 times the Bulk of the Powder; it follows, that if its Elaſticity in the Inſtant of Exploſion be ſuppoſed to be increaſed in the ſame Proportion with that of the Air laſt-mentioned, it becomes by this means about 1000 times greater than the Preſſure of the Atmoſphere; that is, conceiving it to be contained in that Space only which the Powder occupied before it was fired.

Thoſe who have not been converſant in theſe Experiments, may poſſibly ſuppoſe, that the Elaſticity of the Powder at the Inſtant of Exploſion may be im-

mediately known by the Firſt ſudden Deſcent of the Mercury: But many Circumſtances concur to render this Method impracticable; amongſt the reſt it muſt be remembered, that ſome Air is conſtantly left in the Receiver, which is heated by the Blaſt, and unites its Effects in the Firſt Inſtant with the Action of the Powder: Beſides, the Firſt Deſcent may be varied, by varying the Tube, although all things elſe remain unchanged.

By the Method hitherto deſcribed, it is collected, that the Elaſticity of the Fluid produced from fired Gunpowder, when contained in the Space which was taken up by the Powder before the Exploſion, is about 1000 times greater than the Elaſticity of common Air, or, which is the ſame thing, 1000 times greater than the Preſſure of the Atmoſphere.

But, beſides the Determination of the Quantity of Fluid produced from a given Quantity of Powder, (the Method on which this Deduction is founded) there is another Method of diſcovering the ſame thing, which, though leſs obvious, is yet (as hath been already obſerved) more accurate: That is, by examining the actual Velocities communicated to Bullets by the Exploſion of given Charges in given Cylinders; and this is the Subject of the 7th, 8th, and 9th Propoſitions.

And Firſt, it is evident, that this Examination cannot take place, unleſs a Method of diſcovering the Velocities of Bullets be previouſly eſtabliſhed. Now the only known Means of effecting this was, either by obſerving the Time of the Flight of Bullets through a given Space; or by finding their Ranges when they were projected at a given Angle, and thence computing

puting their Velocity on the Hypotheſis of their parabolic Motion. The Firſt of theſe Methods was often impracticable, and in all great Velocities extremely inaccurate, both on account of the Shortneſs of the Time of their Flight, and the Reſiſtance of the Air. The Second is ſtill more exceptionable, ſince, by reaſon of the Air's Reſiſtance, the Velocities thus found may be leſs in any *Ratio* given, than the real Velocity ſought. Now, to avoid theſe Difficulties, the Author has invented a Method of determining the Velocities of Bullets, which may be carried to any required Degree of Exactneſs, and is no-ways liable to the forementioned Exceptions; for, by this Invention, the Velocity of the Bullet is found in any Point of its Track, independent of the Velocity it had before it arrived at that Point, or of the Velocity it would have after it had paſſed it: So that not only the original Velocity, with which it iſſues from the Piece, is hence known, but alſo its Velocity, after it has paſſed to any given Diſtance; and therefore the Variations of its Velocity from the Reſiſtance of the Air may be alſo aſcertained with great Facility. The Machine for this Purpoſe is deſcribed in the 8th Propoſition, and the Principle it is founded on is this ſimple Axiom of Mechanics; *That if a Body in Motion ſtrikes on another at Reſt, and they are not ſeparated after the Stroke, but move on with one common Motion, then that common Motion is equal to the Motion with which the Firſt Body moved before the Stroke:* Whence, if that common Motion and the Maſſes of the Two Bodies are known, the Motion of the Firſt Body before the Stroke is thence determined. On this Principle then it follows, that the

the Velocity of a Bullet may be diminiſhed in any given *Ratio*, by its being made to impinge on a Body of a Weight properly proportioned to it; and hereby the moſt violent Motions, which would otherwiſe eſcape our Examination, are eaſily determined by theſe retarded Motions, which have a given Relation to them. Hence then, if a heavy Body greatly exceeding the Weight of the Bullet, whoſe Velocity is wanted, be ſuſpended ſo that it may vibrate freely on an Axis in the manner of a Pendulum, and the Bullet impinges on it when it is at Reſt, the Velocity of the Pendulum after the Stroke will be eaſily known by the Extent of its Vibration, and from thence, and the known Relation of the Weight of the Bullet and the Pendulum, and the Poſition of the Axis of Oſcillation, the Velocity with which the Bullet is impinged will be determined, as is largely explained in the 8th Propoſition. Where note, that there is a Paragraph by Miſtake omitted in that Propoſition, which ſhould increaſe the Velocity there found in the duplicate Proportion of the Diſtances of the Points of Oſcillation and Percuſſion from the Axis of Suſpenſion; but this only affects that particular Number, for it was remembered in the Computations of the ſucceeding Experiments, the Numbers of which are truly ſtated.

It being explained how the Velocities of Bullets may be diſcovered by Experiment: The next Conderation is, from thoſe Velocities to determine the Force which produced them.

And the Author thought, the beſt Method of effecting this was by computing what Velocities would ariſe from the Action of fired Powder, ſuppoſing its Force

Force to be rightly aſſumed by the Proceſs in the preceding Part; that is, ſuppoſing the Elaſticity of the Fluid thence ariſing to be at firſt 1000 times greater than that of common Air; for then, by comparing the Reſult of theſe Computations with a great Number of different Experiments, it would appear whether that Force was rightly aſſigned; and if not, in what Degree it was to be corrected.

Preparatory to this Computation, the Author aſſumes in his 7th Propoſition theſe Two Principles:

*1ſt*, That the Action of the Powder on the Bullet ceaſes as ſoon as the Bullet is got out of the Piece.

*2dly*, That all the Powder of the Charge is fired, and converted into an elaſtic Fluid, before the Bullet is ſenſibly moved from its Place.

And in the annexed Scholium he has given the Arguments and Experiments which induced him to rely on theſe Poſtulates, all which is neceſſary at preſent to diſcuſs more at large.

If the Force of Gunpowder was ſuppoſed capable of being determined with the ſame Accuracy and Rigour, which takes place in Subjects purely Geometrical, the Firſt of theſe Poſtulates would be doubtleſs erroneous, ſince it cannot be queſtioned but the Flame acts in ſome Degree on the Bullet after it is out of the Piece.

But it is well known, that in Experimental Subjects no ſuch Preciſeneſs is attainable; for thoſe verſed in Experiments perpetually find, that either the unavoidable Irregularities of their Materials, or the Variation of ſome unobſerved Circumſtance, occaſion very diſcernible Differences in the Event of ſimilar Trials. Thus the Experiments made uſe of

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for confirming the Laws of the Colliſion of Bodies, have never been found abſolutely to coincide either with the Theory, or with each other. The ſame is true of the Experiments on the Running and Spouting of Water and other Fluids, and of the Experiments made by Sir *Iſaac Newton*, for the Confirmation of his Theory of Reſiſtances; in which, though they often differ from each other, and from that Theory by One-twentieth, One-tenth, and even ſometimes One-fifth Part, yet thoſe ſmall Inequalities have never been urged as invalidating his Concluſions, ſince, in Experiments of that Nature, it was rather to be wondered at, that the Difference between the different Trials was ſo ſmall.

And if ſome minute Irregularities are the neceſſary Concomitants of all complicated Experiments, it may be well ſuppoſed, that the Action of ſo furious a Power as that of fired Gunpowder, which viſibly agitates and diſorders all Parts of the Apparatus made uſe of, cannot but be attended with ſenſible Variations; and it in Fact appears, that in the Table of Experiments inſerted in the 9th Propoſition, the Velocities of Bullets fired from the ſame Piece, charged with the ſame Powder, and all Circumſtances as near as poſſible the ſame, do yet differ from each other by One-fiftieth, One-fortieth, and ſometimes more than One-thirtieth, of the Whole; and yet the Author does not conceive, that theſe ſmall Differences are any Exception to the Concluſiveneſs of his Principles; but he preſumes, that had he pretended, without diſcloſing his Method, to have computed the Force of Powder, and the Velocities of Bullets, in different Circumſtances, to a much leſs Degree of Accuracy than

than this, he ſhould have been cenſured, as boaſting of what would have been thought impracticable.

If then the Action of the Flame on the Bullet after it is out of the Piece, is ſo ſmall as to produce no greater an Effect than what may be deſtroyed by the inevitable Variations of the Experiments, the neglecting it intirely, and ſuppoſing no ſuch Force to take place, is both a convenient and a reaſonable Procedure: For indeed, without the Aſſumption of Poſtulates of this kind, it were impoſſible to have proceeded one Step in Natural Philoſophy, ſince no Mechanic Problem hath been ever ſolved, in which every real Inequality of the moving Force hath been conſidered.

Now what induced the Author to ſuppoſe, that this Poſtulate (though not rigorouſly true) might be ſafely aſſumed, was the Conſideration of the ſpreading of the Flame by its own Elaſticity, as ſoon as it eſcapes from the Mouth of the Piece: For by this means he conceived that the Part of it which impinged on the Bullet might be ſafely neglected, although the Impulſe of the intire Flame was a very remarkable Force.

With regard to the Second Poſtulate, "That all "the Powder is fired before the Bullet is ſenſibly "moved from its Place;" it is incumbent on the Author to be ſtill more explicit, as this SOCIETY did ſome time ſince appoint a Committee for examining this very Poſition, who, after making a great Number of Experiments, have determined, * *That all the Powder is not fired before the Bullet is ſenſibly*

* See theſe *Tranſactions*, N° 465. p. 172, &c.

*moved from its Place*; and they have at the ſame time aſſigned the Quantities remaining unfired under different Circumſtances.

Theſe Determinations of the Committee are moſt true; but the Author muſt obſerve, that from the Experiments recited by them, and the Quantity of unfired Powder, which they collected, it may be concluded, that in a Barrel of a cuſtomary Length, charged with the uſual Quantity of Powder, the Deficiency of Velocity occaſioned by the Powder remaining unfired will be ſcarcely ſenſible; and in the ſhorteſt Barrel ever uſed by the Author, where the Space the Bullet was impelled through was not Five Inches, and where of courſe this Deficiency of Velocity ought to be the greateſt, it cannot amount to One-thirtieth Part of the Whole; and conſequently this Poſtulate, though not rigorouſly true, may yet be ſafely aſſumed, in the inveſtigating the Effects of Powder. But before this is more particularly examined, it is neceſſary to explain the Opinions, which have formerly taken place on this Subject.

Thoſe who have hitherto wrote on the Manner in which Powder takes Fire, have ſuppoſed it to be done by regular Degrees; the Firſt Grains firing thoſe contiguous, and they the next ſucceſſively; and it has been generally thought, that a conſiderable Time was employed in theſe various Communications: For Mr. *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from ſome Experiments made at *Petersburgh*, that the greateſt Part of the Charge eſcapes out of the Piece unfired, and that the ſmall Part, which is fired, does not take Fire till it is near the Mouth. Many Theories too have been com-

compoſed on the Time of the Progreſs of the Fire amongſt the Grains, and the different Modifications which the Force of Powder did thence receive; and it has been generally conceived, that the proper Lengths of Pieces were determinable from this Principle; "That they ſhould be long enough to give "Time for all the Powder to fire."

But the Author being ſatisfied, that no ſuch regular and progreſſive Steps could be obſerved in the Exploſion; and having found, that by loading with a greater Weight of Bullet, and thereby almoſt doubling the Time of the Continuance of the Powder in the Barrel, its Force received but an inconſiderable Augmentation; and finding too, that doubling or trebling the uſual Charge, the Powder thus added always produced a correſpondent Effect in the Velocity of the Bullet; and diſcovering likewiſe in a Piece near Four Feet in Length, charged with an uſual Charge of Powder, that the Velocity communicated to the Bullet, during the Firſt Three Inches of its Motion, was full half the Velocity which it acquired in its whole Paſſage through the Barrel, and that the Elaſticity or Force of the Powder, in the Firſt Three Inches of its Expanſion, was, at a Medium, near Eight times greater than in the laſt Two Feet of the Barrel; he concluded from all theſe Circumſtances, that the Time employed by the Powder in taking Fire was not neceſſary to be attended to in theſe Computations; but that the whole Maſs might be ſuppoſed to be kindled, before the Bullet was ſenſibly moved from its Place.

And the Experiments reported by the Committee are the ſtrongeſt Proofs, (as far as they extend) that

Powder is not fired in the progreſſive Manner uſually ſuppoſed; for when the ſhort Barrel was charged with 12 *dwt.* and with 6 *dwt.* reſpectively, the Quantity of Powder which was collected unfired from 12 *dwt.* did not exceed by 3 Grains, at a Medium, what was collected from 6 *dwt.* although the Bullet was a leſs Time in paſſing through the Barrel with 12 *dwt.* than with 6 *dwt.* it having a leſs Way to move; conſequently the Quantity remaining unfired of the 6 *dwt.* did not continue unfired for want of Time, ſince, when the Piece was charged with 12 *dwt.* the additional 6 *dwt.* was conſumed in a ſhorter Time.

And again, when the Barrel was ſo ſhortened, that the Bullet, being placed cloſe to the Wad, lay with its outer Surface nearly level with the Mouth of the Piece, ſo that it had not more than half an Inch to move before the Flame would have Liberty to expand itſelf; yet, even in this ſhort Tranſit of the Bullet, only 2 *dwt.* 1 $\frac{1}{2}$ *gr.* was collected unfired, at a Medium; which is about $\frac{1}{6}$ of the whole Charge, or, if properly reduced, not more than $\frac{1}{12}$ of the Charge: An obvious Confutation of the gradual Firing of the Powder in its Paſſage through the Barrel, and an eaſy Proof, how ſmall an Error will be occaſioned by ſuppoſing the whole Charge to fire inſtantaneouſly, ſince the Error in the Velocity of the Bullet, ariſing from a Deficiency of $\frac{1}{12}$ of the Charge, is $\frac{1}{24}$ of that Velocity only.

I ſay, that the $\frac{1}{6}$ of the Charge, which remained unfired, amounts to no more than $\frac{1}{12}$ when it is reduced at it ought. This Reduction is founded on the

the other Experiments reported by the Committee, and on the Circumſtances of thoſe Trials on which the Author founded the preſent Poſtulate. The Author has ſuppoſed the Powder, on which he reaſons in this Treatiſe, to be of the ſame ſort with that made for the Service of the Government, a Parcel of which he was favoured with by Mr. *Walton*. But this he chiefly kept for a Standard, and generally uſed other Powders, which, on Examination, he found to be of equal Force. Theſe Powders were of a very ſmall and even Grain, and the Committee have found, that by ſifting the Government Powder, and making uſe of the ſmaller Grains, the Quantity remaining unfired was leſs, at a Medium, in the *Ratio* of 5 to 3, than when it was uſed without ſifting.

And again, it was found by extracting the Saltpetre from the Powder collected unfired, that there was leſs Saltpetre contained in it than in real Powder, and this nearly in the *Ratio* of 9 to 7: Theſe Two Proportions compounded make the Proportion of 15 to 7, and in this Proportion muſt the Quantities of Powder collected unfired be reduced, in order to determine the Quantities of real Powder remaining unfired, in ſimilar Experiments made by the Author.

And from hence it follows, that in the Experiments made with a Barrel 5 $\frac{1}{5}$ Inches in Length, where the Ball had not 3 Inches to move, and where the Irregularity ariſing from the Powder unfired ought to have been the moſt ſenſible, the Quantity of real Powder collected unfired from a Charge of 12 *dwt.* would have been no more than 16 Grains at a Medium, or $\frac{1}{18}$ of

of the whole Charge; and it being found by Experiment, that the Velocities of Bullets placed in the ſame Situation vary in the ſubduplicate Proportion of the Charges, the Deficiency of Velocity ariſing from the Loſs of the $\frac{1}{18}$ of the Charge would be about $\frac{1}{36}$ of the whole Velocity only, which, in the preſent Caſe, is not $\frac{2}{10}$ of an Inch in the Chord of the Arch deſcribed by the Pendulum meaſuring the Velocity, and is a leſs Difference than what frequently occurs in the exacteſt Repetition of the ſame Experiments.

Other Circumſtances occur, which reduce the Inequality ariſing from the unfired Powder ſtill lower; but it is thought, that this is fully ſufficient to juſtify the Poſtulate in Queſtion, eſpecially as, in all Caſes of real Uſe, the Length of the Barrel in proportion to the Quantity of the Charge will be much greater than in the preſent Inſtance: Whence the Author preſumes, that, in computing the Velocities communicated to Bullets by the Action of Powder, it may be ſafely ſuppoſed, that the whole Charge is fired before the Bullet is ſenſibly moved from its Place; at leaſt there is no Foundation, from the Experiments made on this Subject by the Committee, to ſuſpect that when ſmall-grained Powder is made uſe of, any greater Irregularity will ariſe from the Application of this Suppoſition, than what would otherwiſe take place from the Intervention of unavoidable Accidents.

It has been thought neceſſary to diſcuſs more at large theſe Two Poſtulates, becauſe the laſt of them being almoſt in the very Words of one of the Queſtions propoſed to be examined by the Committee of this

this SOCIETY, and having by them been determined in the Negative, thoſe who have not attended to this Subject might ſuppoſe, that thereby the Author's Principles were intirely overturned: Now this would be a great Injuſtice to him, ſince he has not relied on this Poſtulate as rigorouſly true; for he knew, and has himſelf taken notice in the preſent Propoſition, that ſome of the Powder eſcapes unfired; and he has there made ſome Conjectures on the Cauſe of it; but, without inſiſting on the Reality of thoſe Conjectures, he adds, that, " Be that as it may, the Truth of our " Poſition cannot in general be queſtioned."

And though it appears from what has been already ſaid, that the Experiments recited by the Committee rather confirm than invalidate the general Senſe of that Poſtulate; yet it is but Juſtice to own, that they are a full Confutation of the Conjectures of the Author in relation to the Cauſe why ſome Part of the Powder comes out unfired; for the Author has ſuppoſed, after *Diego Uſano*, that the Part which thus eſcaped, was ſcattered in the Barrel, and not rammed up with the reſt, or elſe that it was of a leſs inflammable Compoſition: But the Experiments made on this Occaſion intirely deſtroy this Suppoſition.

As this, or any other Conjecture on the Cauſe of this Accident, (for it plainly appears not to be for want of Time only) has nothing to do with the general Reaſoning of the preſent Treatiſe, it is not neceſſary to enter into it in this Place; but it may not be improper to mention, that, on computing the Quantities of Powder collected from different Charges, one of the Committee was led to conjecture, that what was thus collected was only Parts

of

of Grains that had been fired, but were extinguiſhed by the Blaſt before they were intirely conſumed. This Conjecture is ſtrengthened by the extreme Minuteneſs of the Particles of all the Powder which was collected, and from the Deficiency of the Saltpetre found in it on Examination: It may be added too, that the Author, by gradually heating a Parcel of Powder, hath ſet it on Fire, and blown it out again, for at leaſt a Dozen times ſucceſſively; and he will undertake to repeat the Experiment at any time, if it ſhould be doubted of.

The Poſtulates hitherto diſcuſſed are preparatory to the 7th Propoſition. That Propoſition is employed in computing the Velocity which would be communicated to a Bullet in a given Piece by a given Charge of Powder, on the Principles hitherto laid down, that is, ſuppoſing the Elaſticity of fired Powder to be at firſt 1000 times greater than that of common Air.

In the 9th Propoſition theſe Computations are compared with a great Number of Experiments, made in Barrels of various Lengths, from Seven Inches to Forty-five Inches, and with different Quantities of Powder, from 6 *dwt.* to 36; and the Coincidence between the Theory and theſe Experiments is very ſingular, and ſuch as occurs in but few philoſophical Subjects of ſo complicated a Nature.

By this Agreement between the Theory and the Experiments, each Part of the Theory is ſeparately confirmed; for by firing different Quantities of Powder in the ſame Piece, and in the ſame Cavity, it appears that the Velocities of the Bullet, thence ariſing,

ariſing, are extremely near the ſubduplicate Proportion of thoſe Quantities of Powder, and this independent of the Length of the Piece: Whence it is confirmed, that the Elaſticity of fired Powder in various Circumſtances, is nearly as its Denſity; and this does not only ſucceed in ſmall Quantities of Powder, and in ſmall Pieces, but in the largeſt likewiſe, under proper Reſtrictions; at leaſt there are Experiments which could not be influenced by this Theory, where the Quantities of Powder were above 100 times greater than what are uſed by this Author, and in theſe Trials this Circumſtance takes place to ſufficient Exactneſs.

It is preſumed then, that by this Theory a near Eſtimate may be always made of the Velocities communicated to Shells or Bullets by given Charges of Powder; at leaſt theſe Experiments evince how truly the Velocities of ſmall Bullets are hereby aſſigned; and the Author can ſhew by the Experiments of others, that in a Shell of Thirteen Inches Diameter, impelled by a full Charge of Powder, the ſame Principle nearly holds: It is true indeed, that when the Charge is much ſmaller than the uſual Allotment of Powder, there are ſome Irregularities, which are particularly mentioned at the End of the 9th Propoſition, to which Head too, perhaps, muſt be referred the Experiments made by the Committee on the Effect of different ſmall Chambers; but in the cuſtomary Charges, the Velocities of Bullets reſulting from all the Experiments hitherto made, are really ſuch as the Theory laid down in the preceding Part of this Treatiſe requires. And it appears, that theſe Velocities are much greater than what they have been hitherto accounted: And there are Reaſons from the Theory to believe, that in Can-

non-ſhot the Velocities may ſtill exceed the preſent Computation.

The aſcertaining the Force of Powder, and thence the Velocities of Bullets impelled by its Exploſion, and the aſſigning a Method of truly determining their actual Velocities from Experiments, are Points from whence every neceſſary Principle in the Formation or Management of Artillery may be eaſily deduced: Conſidering therefore the infinite Import of a well-ordered Artillery to every State, the Author flatters himſelf, that whatever Judgment may be formed of his Succeſs in theſe Inquiries, he will not be denied the Merit of having employed his Thoughts and Induſtry on a Subject, which, though of a moſt ſcientific Nature, and of the greateſt Conſequence to the Public, hath been hitherto almoſt totally neglected; or, at leaſt, ſo ſuperficially conſidered, as to be left in a much more imperfect State than many other philoſophical Reſearches.

With regard to the Second Chapter of this Treatiſe, relating to the Reſiſtance of the Air, the Author has in his Preface mentioned his Intention of annexing to it a Series of Experiments, on the real Track of Bullets, as modulated by that Reſiſtance: And therefore, as he propoſes to complete thoſe Experiments this Summer, unleſs unforeſeen Accidents prevent him, he chooſes to poſtpone any Account of the Subject of the Second Chapter till that time, when he intends to lay the Reſult of thoſe Experiments before this SOCIETY, in order that any Exceptions or Difficulties relating to them, may be examined and diſcuſſed before they are publiſhed to the World.

The Reader is deſired to correct an Error in the Firſt Paragraph of the 29th Page of the Treatiſe here referred to, where for *Percuſſion* read *Oſcillation*.